

Interactive comment on “Future humidity trends over the western United States in the CMIP5 global climate models and variable infiltration capacity hydrological modeling system” by D. W. Pierce et al.

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Received and published: 5 February 2013

Dear Reviewer 2,

we would like to thank you for your helpful comments on the submitted manuscript. We anticipate being able to take all of them into account in the revision.

A few quick comments on points you have raised that require more than a straightforward text revision:

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Regarding comment 4, yes, my understanding is that if you supply the annual precipitation to VIC then the code does not use the "modified" algorithm with the 90-day averaging window. I can say definitively that people here at SIO who have published papers with VIC have not used this option. However in published papers from other groups I have not seen this detail specified either way, so I cannot be sure what various published works have done with this. My hunch is that since the issue with the 90-day averaging window has not been previously identified (as far as I know), most people probably didn't bother specifying the annually averaged precipitation, since it would require extra processing that would not be motivated unless one knew that it had an effect. However that is just a guess, so I will point out this issue in the revision and note that with most published works it is not clear whether or not the annual precipitation was specified.

Regarding comment 5, the concern originates from the information that NOAA supplies on the GSOD data set, which states, in part: "Summary of GSOD from NOAA: Contains 24-hour observations derived from hourly synoptic reports exchanged on the Global Telecommunications System (GTS) and archived in NCDC's Integrated Surface Dataset (DSI-3505). The 24-hour summary period is generally midnight-midnight UTC. Daily values derived in this fashion may differ significantly from "true" daily data, particularly for precipitation. However, they provide data for a number of locations that are not contained in any other archive and represent the only source of daily updates for others (see <http://www.ncdc.noaa.gov/oa/climate/ghcn-daily/>)." Based on this, we thought it would be prudent to check if this issue was affecting our analysis, which it does not seem to be.

Regarding comment 10, I believe the text may be worded poorly or misleadingly on this subject. If the model RH problems resulted in a *constant* runoff bias, then this work would not have anything relevant to say about runoff *changes* with climate change. However, what we find is that as model-projected climate change progresses over this century, the magnitude of the runoff bias increases, because the global models indicate

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decreasing RH over the century but the VMS algorithms do not (or at least, too weakly). So it is the *change* in runoff bias that is relevant, and which our simple estimate suggests could equate to a reduction in Lees Ferry flow of about 4% by the end of the century. I.e., the 4% is an estimate of the sensitivity given model-estimated climate change in the region, not a constant bias. We certainly agree with you that a constant 4% bias would not be relevant to climate change.

Thank you again for your comments, they will help improve the manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 13651, 2012.

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